

A Deterministic Approach to Causality

by
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A DETERMINISTIC APPROACH TO CAUSALITY

av

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Abstract This thesis summarizes four papers which together defend a deterministic view of causality. The first paper is an explicit defense of a deterministic assumption and argues, in part through a critique of the probabilistic theory advanced by Patrick Suppes, that the concept of causality presupposes determinism. A second paper presents an implicit definition of causality. It is argued that this definition solves some of the classical problems of causality, in particular those concerning causal assymetry, counterfactual conditionals and accidental generalizations. The third paper deals with the problem of confirmation from the point of view developed in the previous paper. Arguments in support of causal hypotheses are explicated as having an eliminative structure. It is shown that the most important classical objections to eliminative arguments can be countered succesfully and that the explication can explain many of our informal intuitions about how various features of empirical evidence bears on the degree of confirmation. The final paper is concerned with the selection of causes from large sets of conditions and with the weighting of causes according to their relative explanatory power. The framework is deterministic, and a comparison is made with probabilistic theories of weighting.		
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Date January 1st, 1984

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To Marita

This thesis is a summary of the following articles which will be referred to in the text by their Roman numerals:

- [I] Causality and determinism. Philosophy of Science 48:591-605, 1981.
- [II] Alternative causes and causal laws. Manuscript, 1983.
- [III] On the confirmation of causal hypotheses. Manuscript, 1983.
- [IV] Explaining differences and weighting causes. Theoria 49:87-111, 1983.

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Lund, December 1983.



I INTRODUCTION

Modern science, particularly natural science but also a large part of social science and the humanities, is hardly conceivable without the concept of causality. Indeed, to a very large extent, scientific knowledge is causal knowledge. The reason for this is the eminent practical and intellectual usefulness of understanding the causal relations between events in the external world. The knowledge that A causes B provides us with the necessary instruments for producing or preventing B, and for predicting B, and it also enables us to explain individual occurrences of B.

This view has been shared by the majority of philosophers since antiquity. Aristotle stated categorically that '...we only have knowledge of a thing when we know its cause' (Posterior Analytics, 71b 30). A different view was taken by Bertrand Russell. In the notorious paper 'On the Notion of Cause', Russell dismissed the law of causality as a 'relic of a bygone age' and claimed that concept of causation had been replaced in the more advanced sciences by the notion of a functional relation.

Russell's claim seems to be based on the possibly correct but misinterpreted observation, that causal terminology has often been replaced by mathematical symbolism in theoretically advanced sciences. But the main reason for this is the difficulty of expressing quantitative causal relations in qualitative causal language. Causal terminology only admits of laws like 'A causes B', and it would be very difficult to formulate, say, Ohm's law $R = U/I$ in this way. But it would be incorrect to say that the functional relation had replaced the causal concept. This becomes clear when one compares the symmetry of an equation with the asymmetry of the law. Changes in the resistance of a simple circuit, for instance, cause changes in the current, but not conversely; we cannot increase the resistance by increasing the current, although we can calculate the change which must have occurred in the resistance from knowledge about the change in current. Clearly, the equation does not express the whole content of the law.

In spite of the central role played by the causal thinking in science, and in spite of enormous efforts by philosophers, the concept of causality has remained puzzling. Although the problems it poses are intertwined in a complex way, it is convenient to group them broadly into four categories.

(a) The first group concerns the nature of the causal connection itself. A causal relation is clearly something more than just succession. My dropping a pencil on the floor is followed by, and causes, a sound, but it was also followed by my coughing, which was causally independent of the pencil's fall. The same is true of regular successions. Some regularities are causal, such as the workers leaving the factory when the factory hooters sound. Others are not, such as the workers in Manchester leaving the factory when the hooters in London sound. Regularities of the second kind are examples of what statisticians call 'spurious correlations'. Another kind of non-causal regularity is the so-called 'accidental generalization'. Everyone who reads this text knows English, but the reading is not a cause of the skill. The difficulty of distinguishing causal from non-causal regularities has been put forth as a problem mainly for those theories of causation which hold that causality consists in regularity, but any adequate theory must be able to deal with it.

A second source of difficulty is the asymmetry of the causal relation. When an individual event *a* causes another individual event *b*, it is generally thought to be impossible that *b* causes *a*. At the level of individual events, the causal relation has a direction. It has been held by many, that this asymmetry is nothing more than a reflection of the fact that the cause must precede the effect in time (e.g. Hume 1748; Suppes 1970), but this is probably incorrect. Causes are frequently simultaneous with their effects, and in some cases where they are not, they are so close in time that no-one but the professional physicist would know that a temporal lag existed. The child knows very well, for instance, that the pressing of a certain button causes the lights to come on, but since the child is not able to notice the time-difference, there must be some other criterion at work. Another problem with this alleged solution is that it defines away the question of why an effect cannot precede its cause. Even if we conceded that the temporal sequence should be included in the definition of 'cause', we may consider the concept of 'quasi-causation' (Dummett 1954) which is defined in the same way except that we drop the temporal requirement. It would still sound distinctly odd to hear that a present event is quasi-caused by something that will happen tomorrow. Furthermore, we can rephrase the question and ask why it is, that we cannot change the past. These questions are very difficult and have been extensively discussed in the literature. (Concerning asymmetry in general see e.g. Mackie 1974. Very promising approaches are Simon & Rescher 1966 and Sanford 1976; concerning backwards causation see Dummett 1954, 1964;

Flew 1954 and for further references Mackie 1974)

A third problem which is central to the understanding of the causal relation is the proper interpretation of so-called 'counterfactual conditionals', i.e. conditional statements in which the antecedents are false. For example, 'if a lump of butter, which was eaten yesterday, had been heated to 100°C it would have melted' or 'if I had struck this match, it would have lighted'. These statements seem to rely on a causal relation between, in the first case, heat and melting of butter, and, in the second case, friction and lighting of matches. However, it is also tempting to use counterfactuals to pin-point the difference between causal and non-causal forms of succession. For instance, the falling of my pencil would have been followed by a sound, even if it did not actually fall, whereas it would not be true that 'if the pencil had fallen at a time when it actually did not, I would have coughed'. Sorting out the relationship between causes and counterfactuals is an important task for an adequate theory of causation.

(b) A second set of problems concern the ways in which causal knowledge is obtained. It is not self-evident that this is a problem that should be viewed a problem of causality. In fact, the majority of modern philosophers who have written on the confirmation of scientific hypotheses have treated all generalizations alike, and few have acknowledged the existence of a problem of the confirmation of causal hypotheses specifically. In the so-called 'confirmation theory' of the logical empiricist tradition, where all laws are construed as universal generalizations, an hypothesis is confirmed to the extent that it is consistent with individual instances. The observation of a black raven, for instance, would confirm the hypothesis that all ravens are black. There are enormous difficulties with this approach, however. For instance, Hempel (1965) noted that the hypothesis that 'all ravens are black' is logically equivalent to the hypothesis that 'all non-black things are non-ravens'. Since the latter hypothesis would be confirmed by the observation of, say, a white swan, and since the two hypotheses are equivalent, a white swan would also confirm the hypothesis that all ravens are black.

A very different approach was taken by Mill (1843) who held that causal statements are normally supported by eliminative arguments. Suppose, for instance, that the only event which is present on all those occasions on which E occurred is C, then C must be the cause of E. Mill formulated five such methods, which he called the 'canons of induction', but they are all

essentially eliminative. These methods have been criticized mainly for the assumptions that they require. Firstly, we must assume that each occurrence of C is caused by something, and secondly we must have a finite list of all the possible causes. (Cf. e.g. Kneale 1949) For these reasons, Mill's methods are not taken seriously by many today.

(c) The relative importance of causes. This problem has to do with the way that a cause is selected from a set of conditions which, at least from a physical view-point, all have a claim as determinants of the event in question. To produce a fire, for instance, we need a combustible material, a heat source and oxygen, but when a house burns down after someone has failed to shield the fire-place, we do not explain the fire by pointing out the abundance of oxygen. Another aspect of the problem is the meaning of statements which claim that among several competing conditions, one is more important than the other, as in the nature-nurture controversies. These questions have not received very much attention from philosophers, in spite of the fact they are extremely important for understanding the nature of scientific knowledge. A possible reason for this neglect is the fact that causal selections play a very limited role in abstract scientific theorizing, the more abstract, the less important the causal selections. In those parts of physics for instance where the value of a variable is quantitatively and precisely determined by a set of other variables, a ranking of these would be pointless. No one would say that volume was a more important determinant of pressure than was temperature. In practical applications, however, the situation is very different.

(d) The nature of events. The problem of the nature of events has gained attention only in last two decades. Hume seems to have regarded events as unproblematic and spoke also unhesitatingly of the 'similarity' of one event to another. The logical empiricist philosophers seem to have regarded the nature of events mainly as a matter of notational convenience. Generic events or event 'kinds' were taken to be reducible to propositional functions or predicates and individual events could be construed as statements or propositions. In recent times many philosophers have argued that it is necessary to posit events as distinct ontological entities. Davidson (1967) for instance argued that if a and b in the statement 'a caused b' are construed as sentences, then the statement must be truthfunctional. Another argument was an application of Quine's argument about the referential transparency of modal contexts. Both of these arguments have later been

criticized by e.g. Mackie (1976) and Horgan (1976), and there is at present no standard view of the matter.

During the history of philosophy, a large number of theories of causation have been proposed. The following list is representative, although it does not claim to be exhaustive.

Causal Powers. The idea of causal powers was held by many philosophers from Aristotle and onwards. When an event a caused another event b, the element that differentiated the causal relation from one of succession was the 'passive power' of an object to be transformed in a certain way, or the 'active power' of an object to transform another object in a certain way. For instance, '...there is a passive power residing in anything acted upon which is the source of its undergoing change through the action of some other thing or through its own activity upon itself.' (Aristotle, *Metaphysics* IX,1,1046a) It is the power of the oil to burn that explains why the oil, but not, say, a rock, burns when heated.

The idea of powers was criticized by Hume in Enquiry on the ground that powers were unobservable and did not correspond to any sense experience. This has been the view of the vast majority of philosophers since then, although there have been attempts to revive the idea (e.g. Harre' & Madden 1975).

Natural necessity. The idea of natural necessity is, in substance, not very different from the powers account. That A causes B means, according to this theory, that B not only succeeds A, but that B is made necessary by A, or that, when A occurs, B cannot fail to occur. Whether this theory can solve any of the problems of causality is difficult to say, because of the obscurity of the idea. If necessity is to be understood in modal terms, the theory would hold that when A occurs, the non-occurrence of B is impossible. However, it is difficult to see how such a claim could be defended except by reference to laws of nature, and then we would have the problem of the corresponding connections in the laws. The basic weakness of this view, as of the powers idea, is that it only relocates the problems. The questions of causal direction, for instance, will transform into questions about the direction of powers or of necessitation, but they will not be solved.

Necessity can also be contrasted against 'accidence'. That a necessary connection obtains between two events would then mean that their co-occurrence

was not an accidental fact. But this distinction seems to be the same as that between 'causal' and 'accidental' regularities described above. Again, the problems are rephrased rather than solved.

Natural necessities were criticized by Hume with the same arguments as those applied to powers. 'There are no ideas, which occur in metaphysics, more obscure and uncertain than those of power, force, energy or necessary connection ... We are never able, in a single instance, to discover any power or necessary connection; any quality, which binds the effect to the cause, and renders the one an infallible consequence of the other. We only find that one does actually, in fact, follow the other.' (1748 section VII, part I) Hume's critique has been enormously influential and few philosophers in this century have taken natural necessities seriously. For those who have (Ewing 1934; Kneale 1949; Fisk 1973; Harre' & Madden 1975) the main reason has been the failure of competing theories to solve the problems mentioned above.

Regularity theory. If Hume's claim is correct, and there never exists any connection between two events A and B, what then is the difference between A's being followed by B and A's causing B? Hume's answer was the regularity with which events of the same kind as A are followed by events of the same kind as B. 'We may define a cause to be an object followed by another, and where all the objects, similar to the first, are followed by objects similar to the second.' (1748 section VII, part II) In this, Hume did not deny that we have an idea of a necessary connection, only that this idea does not correspond to anything existing in the objects. The regularity theory was intended to explain this idea in terms of observable phenomena.

In various guises, the regularity theory has been the dominant view of causation since the middle of the 19th century. It was held, for instance by Mill (1843) and by Popper (1959) and by nearly all philosophers in the logical empiricist tradition (e.g. Berofsky 1971; Goodman 1965; Hempel 1965; Nagel 1961). The theory appealed to these philosophers because of their empiricist leanings, but also because their main interest was scientific knowledge, and the regularity theory seemed to make sense of the scientist's search for laws.

By focusing on laws, modern empiricists improved the theory in one respect. A problem with Hume's formulation concerns the meaning of 'similarity'. For example, 'love alone caused King Edward to leave his throne', but which events

are 'similar' to Edward's love and to his leaving the throne? The natural answer is that the events must be similar in relevant respects, but 'relevance' in this context can hardly mean anything but 'causal relevance', and this would make the analysis circular. By formulating the analysis in terms of laws, however, the concept of similarity is rendered superfluous. If a law states that all events with a certain set of specified properties A are followed by events with the specified properties B, we apply the law to all events which have these properties. The question of similarity never arises.

In spite of its popularity, the regularity theory has, so far, proved unable to solve any of the main problems of causality. Various attempts to modify the theory in terms of sufficient and necessary conditions have also been unsuccessful.

Action theories. Many philosophers have held that causality is essentially an anthropomorphic concept. Thomas Reid, for instance, claimed that the idea of causation and causal powers in inanimate things was derived from that of human agency. But 'The only distinct conception I can form of active power is the idea that an agent can do certain things if he wills'. In Reid's view, 'the relation between a cause and its effect' is 'similar to that which is between us and our voluntary actions' (1788, Essay I, chapter V).

Modern variants of the action approach have generally concentrated on the relation of means to ends (Collingwood 1938; Gasking 1955; von Wright 1971, 1974). Thus Collingwood refers to causes as 'levers' by means of which we can produce or prevent the effects. In a similar vein von Wright writes 'By doing certain things we bring about other things. For example, by opening a window we let fresh air into the room (bring about ventilation)...What we thus bring about are the effects of our action. That which we do is the cause of those effects.' (1971, p. 66) That A causes B thus means that by doing A we can bring about B.

von Wright claims that this theory solves the main problems of causation. Take for instance the problem of accidental uniformities. It is quite likely that 'Everyone who reads this thesis knows Swedish' is true, but it would not be causal, for I cannot bring it about that someone knows Swedish by making him read the thesis. But this solution is entirely illusory. To see this, we need only ask why we cannot bring about a knowledge of Swedish in this way. Or suppose that I test the causal nature of the statement by making someone

read the thesis, and it turns out that that person also knows Swedish. Have I then supported the hypothesis? von Wright may here say that this person knows english, but that my action did not bring it about. But in that case, the concept of causality is merely concealed in the concepts of 'doing' and 'bringing about'.

Counterfactual theory. As noted above, a feature of causal relations which is not shared by accidental uniformities is their ability to support counterfactuals. If it were possible to analyze counterfactuals without referring to causal relations, it might be possible to analyze causality in terms of counterfactuals. Such a theory has recently been proposed by Lewis (1973).

Lewis' theory is based on a particular analysis of counterfactuals. To put it briefly, the statement

if I had not dropped the egg, it would not have broken

is true iff

there is a possible world W in which the egg was not dropped and did not break, such that W is more similar to the actual world than any world in which the egg broke but was not dropped

If this counterfactual is true, then the egg's breaking is, in Lewis' terms, causally dependent on the dropping of the egg.

There are several problems with this approach which are of a technical nature (Cf. Kim 1973), but there is also an obvious difficulty involved in the very nebulous concept of 'similarity'. To make this a useful concept, 'similarity' must be defined in a way that does not presuppose any judgements of relevance which can be traced back to causal relevance. It seems highly unlikely that this will ever be possible, and there is a clear danger that the traditional problems of causality will crop up in a new form. Nevertheless, the possibility of analyzing causality in terms of counterfactuals has attracted many writers (see e.g. Mackie 1974; Nordenfelt 1981; Sahlin 1983)

II DETERMINISM AND INDETERMINISM

Although Hume and his followers among 20th century positivists define causes as sufficient conditions, most philosophers have been well aware that few causes are truly sufficient for their effects. Indeed, it could be argued that no causes are really sufficient, and that all empirical laws have exceptions. This is not necessarily very damaging to the empiricist position, however, for there are (at least) two possible escape routes.

The one most frequently used is what we may call the determinist view. This is roughly that a cause is in some sense a 'part' of a truly sufficient condition. Thus, Mill notes that a causal relation holds 'between a consequent and the sum of several antecedents; the concurrence of all of them being requisite to produce... the consequent.' and 'In such cases it is very common to single out one only of the antecedents under the denomination of Cause.' (1843, Bk III, ch. v, #3) What we call a cause is generally only a small part of 'the sum of several antecedents'. A similar approach was taken by J.L. Mackie (1965) (Cf. also Marc-Wogau, 1962). Mackie defined a cause as an INUS condition, by which he means an insufficient but necessary part of an unnecessary but sufficient condition. The reason for calling these views 'deterministic' is, that if a cause is defined as a part of a sufficient condition, every cause will entail the existence of a sufficient condition and the truth of a deterministic law.

This does not in itself imply determinism in the metaphysical sense. Firstly, this view of causes is consistent with the existence of completely uncaused events; it is only the caused events which must be sufficiently caused. Still, most critics of determinism have been content to postulate the existence of events for which no sufficient cause exist. Indeterminists do not generally claim that there are events for which no causes at all exist. Thus, although the deterministic view of causes only entails a 'limited determinism', it will in practice be indistinguishable from universal determinism.

Secondly, the deterministic view of causes should be understood as saying, that if there are causes, then there must be sufficient causes; it does not entail the existence of causes. This is important for methodological reasons, for if convincing arguments for indeterminism were produced, it would mean

that the traditional concept of causation would have to be abandoned. (Cf. Earman 1976)

The second way of accounting for non-sufficient causes within the framework of empiricism and the regularity theory is the statistical or probabilistic approach to causality. A natural starting point for such a theory would be a probabilistic association or correlation. Even if the cause does not universally entail the effect, it might seem reasonable to require that the cause at least raise the probability of the effect. A minimum requirement for the assertion that A causes B would then be that $P(B|A) > P(B)$.

There are two problems with this definition, however. The first is that (when the probabilities involved are greater than zero) A raises the probability of B exactly when B raises the probability of A. This will make it impossible to account for the direction of the causal relation and to distinguish between A's causing B and B's causing A. This difficulty can be overcome by requiring that the cause be earlier than the effect. The second difficulty is that a statistical association between A and B can arise because both are caused by some third variable X. This is known in statistics as spurious correlations. A classical example is the correlation between child birth frequency and stork frequency in 19th century Prussia. The third event in this case is the difference between rural and urban regions. Statisticians can often overcome this difficulty with the help of so-called 'partial correlations', which means that the disturbing 'third' factor is kept constant. This has led a sociologist to 'suggest a clearcut definition of the causal relationship between two attributes. If we have a relationship between 'x' and 'y'; and if for any antecedent test factor the partial relationships between x and y do not disappear, then the original relationship should be called a causal one'. (Lazarsfeld 1955, p.125)

Similar ideas have been expressed by Good (1961), Suppes (1970) and Salmon (1980). The most elaborate version is that of Suppes (1970) who defines a cause as a probability-raising event:

At is a prima facie cause of Bt' if and only if

- (i) $t < t'$
- (ii) $P(At) > 0$
- (iii) $P(Bt'|At) > P(Bt')$

(Suppes 1970, p. 12)

In order to overcome the difficulty posed by spurious correlations, Suppes then defines a spurious cause. The basic idea is that A is a spurious cause of B, if there is a third event C such that the statistical association between A and B disappears when the probabilities are conditionalized on C. The association between birth rate and stork frequency, for instance, would vanish if geographical area was held constant. A genuine cause, finally, is *prima facie* cause that is not spurious. (Suppes 1970, pp. 21 - 28)

This theory has its own difficulties, however. For instance, as argued in Hesslow (1976), causes may sometimes lower the probability of their effects. A possible example is the use of birth control pills which, according to current medical opinion, may cause blood clots. But these pills also eliminate the risk of pregnancy, which is a more efficient cause of blood clots. It appears, therefore, that pills could lower the probability of blood clots. (Several writers have commented on this argument. See Eells & Sober 1983, Humphreys 1980, Otte 1981, Rosen 1978, Sahlin 1983 and Salmon 1980. Only Rosen's argument will be considered here.)

In a defense of the probabilistic theory Deborah Rosen (1978) attempted to solve this difficulty by relativising the causal statement on background knowledge. Thus, there might be a subpopulation P in which pills do raise the probability of blood clots. This might be true in a population with good medical facilities for preventing thrombosis in pregnant women, for instance. There are good reasons for rejecting this solution, however. Firstly, it would entail that the causal relation only obtains in the population in which the correlation occurs. But if there is a causal relations, one would expect it to hold in all populations, although it may be masked by various statistical effects. Secondly, this solution has the effect of making causal statements relative to a certain body of knowledge. What causes what would then depend on the state of our knowledge. This is a strongly counter-intuitive consequence.

A second argument advanced against the probabilistic theory in [I] is the impossibility of inferring individual causal relations from statistical associations. Suppose that we have a light which is controlled by two switches, so that turning switch a (A) and circuit a's being intact (A*) is sufficient for the light's coming on (L), and the analogous event B & B* is also sufficient for L. A will of course be a cause of L according to the probabilistic theory and so will B. But in the individual case where A occurs together with B and

B*, but where A* is absent, we would not say that A caused L. A is effective only together with A*. It might be thought that this problem could be solved by relativizing the probability statements on A* and -A*, but in many cases such complementary factors are unknown, and it is the whole point of the probabilistic approach that one should not assume the existence of such factors.

It was also argued in [I] that many arguments in support of causal relations in science have an eliminative structure, that can only be made sense of, if determinism is assumed. (For a critique of this argument, see Katz 1983 and Suppes 1984.)

III THE NATURE OF THE CAUSAL RELATION.

The basic idea in [II] is to provide an implicit definition of the causal relation. It is argued that three conditions are sufficient to characterize the causal relation, in the sense that they can generate all true causal statements. The first condition is the deterministic assumption:

- (D) For any event A occurring at time t, there is at every time preceding t a sufficient cause of At.

The second 'universality' condition is simply the classical empiricist regularity assumption:

- (U) If A causes B, then every occurrence of A is followed by an occurrence of B.

The third condition is based on the concept of an 'alternative cause'. Given the hypothesis that A causes B, an alternative cause with respect to this hypothesis is, in intuitive terms, a sufficient cause of B that is distinct from A.

To see how this concept may be helpful in understanding the nature of the causal connection, consider the following examples: All men who regularly take their wives' birth control pills avoid pregnancy; All samples of table salt which have had dissolving spells cast upon them dissolve in water. If these generalizations were advanced as arguments for causal relations, the common sense objection would be that they are parasitic upon other causal relations. Men will not become pregnant for other reasons even if they do not take pills, and table salt dissolves in water because of physical properties which act irrespective of any spells. In short, in both cases there are always alternative causes present.

A requirement suggested by this is that, if A causes B, then A must be followed by B in cases where there are no alternative causes present. Thus, if we took a sample of something which does not normally dissolve in water and this substance did dissolve after a dissolving spell, we would have evidence that the spell could cause dissolving. This idea is captured in the following relevance requirement:

(R) If A causes B, then there is a pair of time-points s and t such that
As and Bt and $\neg \text{alt}(AB)t$.

Since (R) is part of an implicit definition, it is pointless to criticize it on account of its circularity. It might be thought, however, that it must be impossible to determine if (R) is satisfied. In order to show that A causes B I must first establish the truth of a statement involving alternative causes, and these require knowledge of further alternative causes etc. But this is incorrect (see below).

The relevance requirement provides us with natural answers to the puzzling examples of causal assymetry discussed in the literature. Take for instance the case of the flag-pole introduced by Bromberger (see Hempel 1965). If we regard the position of the sun as fixed, there will be a universal connection between the length of the flag-pole and the length of its shadow, but we would not say that the length of the shadow causally determines the length of the flagpole. We can easily show that the pole is causally relevant to the shadow, by lengthening the pole on an occasion where there is no alternative reason for expecting the shadow to change. Since the shadow will become longer, we conclude that the pole is relevant to the shadow. But the argument cannot be reversed, for how could we change the shadow on an occasion where there was no alternative of a change in the length of the pole. The shadow could be changed by bringing in another object, but this would not change the length of the flagpole. The shadow could also be changed by changing the flagpole, but this would introduce an alternative cause.

A solution to the problem of causal assymetry also gives a natural solution to the problem of counterfactuals. Take Goodman's (1965) standard example:

(i) If the match had been struck, it would have lighted.

seems to be true, because in a context in which (i) is likely to be uttered, there obtained conditions (m is dry, there was oxygen present etc.) which together with the counterfactual assumption that m was struck and together with a causal law entails that m lights. But it is also a law, that all matches which are struck, are surrounded by oxygen and do not light are not dry. Thus, we could also claim that

(ii) If the match *m* had been struck, it would not have been dry.

But we not ordinarily regard (ii) as true. This was problematic for Goodman, because he assumed the classical regularity theory of causality, and this theory is unable to account for causal directions. It is clear that striking a match can cause its lighting but not its dryness. Given a solution to the problem of the causal direction, Goodman's problem disappears.

IV CONFIRMATION OF CAUSAL HYPOTHESES

The purpose [III] is to show that it is possible to confirm a causal hypothesis when defined as above, without utilizing knowledge about other causal relations. It is argued that causal hypotheses are typically confirmed by eliminative arguments, similar to the 'canons of induction' defended by Mill (1843), and that these arguments need not be subject to the criticism which has traditionally been levelled against Mill (cf. above). Although the analysis can be developed in formal terms, I will here give only an intuitive summary.

Let us imagine the following simple machine. It has a button, a bell and a light. The function of the light is to mark a trial, and when the light is on, we may press the button and await the sounding or the silence of the bell. Our goal is to determine if the sinking of the button causes the sounding of the bell.

Consider now the following three situations.

- (a) We press the button in the first five trials, and each time we hear the bell sounding.
- (b) We wait for ten trials and note that the bell is silent in each one. Then we press the button five times and hear the bell sounding each time.
- (c) We wait for ten trials and note that the bell sounds in six of them. Then we press the button five times and hear the bell sound each time.

In all three situations we have five positive instances, but the cases differ markedly in their ability to convince us of a causal connection between the sinking of the button and the sounding of the bell. Case (b) is highly convincing. Case (c) also supports the hypothesis, but leaves a much greater room for doubt. The common sense reflection would be that, since the bell sounded in six of the first ten 'control' trials, it is possible that it would have sounded each time we pressed the button even if we had not done this. Case (a) may be considered as supporting the hypothesis, but it is very much weaker than (b) and (c). Now why does the empirical support vary so much between these cases, in spite of the fact that the number of positive instances is the same?

The answer seems to lie in the varying information about what happens when

the button is not pressed. In (a) we know nothing at all about this; in (b) the absence of button-pressing is followed by the absence of bell-sounding; in (c) the absence of button-pressing is sometimes followed by bell-sounding and sometimes by silence. A natural interpretation of this is the following: we assume that the sounding of the bell is always caused by something, either by the pressing of the button or by some unknown cause. This unknown cause is what we have termed an alternative cause above. In a particular trial in which both button-pressing and bell-sounding occurred, we cannot know for certain if the sounding was caused by the sinking of the button or by an alternative caused, only that it was caused by (at least) one of these events. A positive instance per se cannot help us to make the choice. But if we observe the machine during a large number of trials, in which the button is not pressed, then we will know that each sounding of the bell must be caused by an alternative cause (whatever it may be). This enables us to determine how frequent alternative causes are; if the bell seldom sounds when the button is not pressed, alternative causes must be rare; if the bell sounds frequently, when the button is not pressed, alternative causes must be common. Suppose that the first case holds, so that we have good reasons for believing that it is very improbable, in a given trial, that the machine itself (by its internal wiring, or a ghost or whatever) sounds the bell. If we then press the button and hear the sounding of the bell, it will be improbable that an alternative cause has occurred. But, since the sounding of the bell was caused by something, it will be correspondingly probable that the alternative is true, that is, that the pressing of the button caused the sounding of the bell.

Note that this argument does not presuppose that we are able to identify any alternative causes. The objection to the implicit definition of causality presented above, that it is circular or that it cannot be applied without prior causal knowledge, therefore must be wrong. We may also note that the traditional objections against the eliminative methods of induction developed by Mill (1834) are invalid. We do not need an exhaustive list of the possible causes of an effect in order to eliminate all alternatives but one.

V CAUSAL SELECTION

In most scientifically interesting cases, an event is not caused by one but by an large number of conditions. Yet, when asked about the cause of a particular event, we do not usually mention every condition. Instead we single out one of them as the 'most interesting', the 'decisive', the 'basic' etc. cause. In other cases, we may be willing to consider several conditions, but we rank them according to their 'relative importance'. One wonders if such selections are arbitrary, or if there is a clear meaning to be attached to them.

The basic point of [IV] can be made simply. Suppose that we have have three conditions which are individually necessary and jointly sufficient for the effect E.

$$\left. \begin{array}{l} C_1 \\ C_2 \\ C_3 \end{array} \right\} \longrightarrow E$$

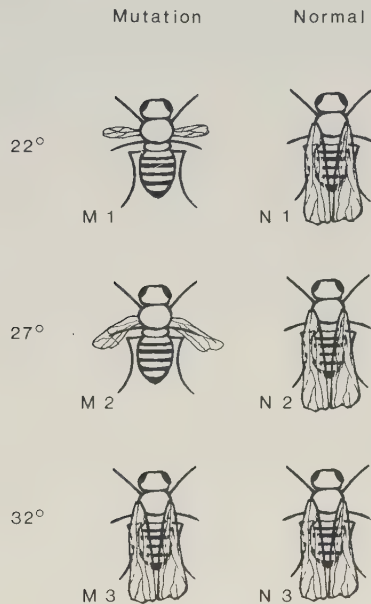
Now, if this was all we had, it would indeed seem natural to say that any selection from these three conditions would be arbitrary, and any claim that one condition was more important than the others would be meaningless. But suppose that we have another case, another individual, where only two of the three conditions are present and where the effect, therefore, is absent.

$$\left. \begin{array}{l} -C_1 \\ C_2 \\ C_3 \end{array} \right\} \longrightarrow -E$$

Suppose now that we are asking, not why E occurred in the first case, but why there was a difference between the two cases, that is why E occurred in the first case, when it did not in the second. When the question is rephrased in this way, C_1 takes on a special significance, because C_1 is the only condition that can explain the difference between the two cases.

Consider the stylized fruit flies in fig. 1, and let us first consider those that are designated M 1 and N 1. N 1 is a genetically normal fly, and M 1 is a mutant. It is assumed that they have been brought up under identical

environmental conditions. If we now ask the question 'Why does M 1 have such short wings?', the answer seems obvious. The cause is genetic - M 1 is a mutant. But suppose that we had never seen N 1, and that M 1 had only been observed together with the flies M 2 and M 3, both of which are genetically identical to M 1 but have been raised in higher temperatures (27°C and 32°C respectively, as compared to 22°C for M 1). If we look at these and put the question again, 'Why does M 1 have such short wings?', the answer will again seem obvious, although it will now be a different one. The wings are short because M 1 has been raised at low temperature.



In explaining the short wings, we seem to be making a comparison between this fly and a different one. We may be unaware that we are making this comparison, but it is the most natural way of making sense of the fact that the answer to our question varies with the object of comparison.

The thesis of [IV] is that all causal explanations involve comparisons of the kind described here, and that causes are selected or deemed important to the extent that they explain a difference between the object under consideration and a set of objects of comparison. This view of causal selection is

consistent with many of the alternative suggestions which have been proposed in the literature. Indeed, it treats them as reducible to what I believe to be the more fundamental principle and explains them as reflecting different ways of choosing the objects of comparison.

(a) Perhaps the most popular view is that the conditions which are singled out are those that are 'abnormal' or 'non-standard' (see e.g. Hart & Honore' 1959; Nordenfelt 1981). This is equivalent to picking out those conditions which explain why an individual differs from what is normal, i.e. the objects of comparison are chosen as the normal objects.

(b) We sometimes pick out 'precipitating' causes: the spark that ignites the explosion (see e.g. Nordenfelt 1981) This is equivalent to explaining the difference between an object now and the same object at an earlier time.

(c) Some philosophers have argued that it is impossible to separate assignments of causality from those of blame, because we sometimes pick out as the most important those conditions which should not have occurred. (Cf. Dray 1957) This is equivalent to making a comparison, not with what is normal, but with what is 'appropriate'.

(d) It is common, particularly in theoretically advanced sciences like physics or economics, to use as objects of comparison purely hypothetical states, idealizations which are defined by an abstract theory. It is not explained in Newtonian physics, for instance, why an object moves exactly as it does, but only why its trajectory deviates from a state of 'uniform motion'. An economist may similarly explain why the economy deviates from a model of perfect equilibrium.

It is further shown in [IV] that this theory of causal selection can be developed into a quantitative measure of relative explanatory power. The only previous measure of explanatory power in the literature has been suggested by Nagel (1961) and developed by Martin (1972). According to these authors, a condition C_1 is more important than another condition C_2 for the effect E, if C_1 gives E a higher probability than C_2 does. It is shown in [IV] that under certain conditions the two measures are equivalent.

An important consequence of this view of causal selection is that all selections, although by no means arbitrary, are relative to a certain choice

of objects of comparison. This in turn is important for many systems of classification which are based on causal weightings and for questions of causal transitivity (cf. Hesslow 1981b, 1984).

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